So far is it from being true that I have represented hovering as an accomplishment of wingmanship which requires little exertion, that I have asserted with emphasis the exactly opposite doctrine—that it is a specially difficult operation, requiring very often great exertion, and always requiring special muscular effort.

It is evident, however, that Mr. Guthrie is still ignorant of the facts which have to be explained. In the passage which he misquotes I am not stating any theory; I am stating a fact which I have seen over and over again. It is a fact beyond all question that a kestrel can maintain itself hovering in a strong horizontal air-current, with no other muscular exertion than that which is required to keep its wings and body at th right angle. I have seen it done a hundred times in level countries, when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground.

One of the first and most fundamental facts to be admitted and accounted for in the flight of birds is, that perfectly horizontal air-currents have a powerfully sustaining effect upon vane surfaces, which are presented to them as birds' wings are presented. "Hovering" and "soaring" are only to be explained when this fact is seen and admitted. ARGYLL

Inverary, Argyllshire, July 30

Exhibition of Specimens and Apparatus at British Association Meetings

I AM anxious to draw the attention of the readers of NATURE to the arrangements to be made this year at the British Association meeting (for the first time) for the reception of specimens and apparatus illustrating papers or short communications made to the sections. The provision of a room for this purpose—a kind of temporary museum—has during the last four years been recommended by the committees of Sections C and D, several times, and this year the experiment is to be made. Those who have promoted this plan are naturally anxious that it should be a success. I would therefore appeal to the secretaries of the various sections to assist in initiating this new feature of the meeting, by endeavouring, as far as possible, to secure from the authors of papers objects which illustrate their communications; such objects to be deposited during the week of meeting in the room provided by the Council. This room will be open to inspection under the same regulations as the sectional meeting rooms, and the objects deposited will be carefully ticketed and arranged, and, where necessary, placed under glass cases.

From Section A we may expect physical and astronomical apparatus and models; from B, new chemical products and specimens of apparatus illustrating new processes; from C, geological specimens of rarity or new to science; from D, zoological and botanical specimens, anatomical preparations, for the exhibition of which microscopes will be provided, and also ethnological specimens; from E, maps and geographical models; from F and G, models or machinery not too large for a room.

It is necessary to mention that objects exhibited must be in illustration of some communication (however short) to one of the sections, in order that they may thus be sanctioned by the committee of such section.

By the co-operation of the sectional secretaries with the members of the committee appointed to superintend the arrangements of this room or repository, we ought to succeed in adding an important and valuable feature to the scientific interest of the meetings of the Association. E. RAY LANKESTER

A Waterspout at Milford Haven

THE enclosed account of a waterspout which was sent to me by one of our telegraphic reporters may perhaps be of interest to your readers. ROBERT H. SCOTT

Aug. 1

"St. Ann's Head, Milford Haven, July 28

"Sir,—The waterspout mentioned in this morning's report was observed yesterday at 4.50 P.M., about a mile outside the port, following in the wake of a squall. Its course lay about N.E., and the progressive movement was judged to be between twenty-five and thirty knots per hour. Its diameter at the base was about 40 ft., and the direction of the whirl from left to right, or with the hands of a watch. The lower portion was well defined, but the middle and upper portions were not so distinct; in fact, the connection with the clouds above, although undoubtedly existing, could not be discerned from our point of view. The sea immediately under it was greatly agitated and white with foam, the spray ascending in a spiral form. Thunder was heard with the squall that preceded it, and the wind veered from S. to S.S.W., although it backed to S. again afterwards. (Signed) JOHN C. WALKER

"R. H. Scott, Esq."

Periodicity of Rainfall

MY attention has been recalled to the letter (vol. viii. p. 547) of my old friend Mr. Meldrum, dated Sept. 15 last, upon the above subject, by its recent republication in a Barbados newspaper. I had intended at the time to examine whether his objections to my statements were valid, but absence from the island and other occupations interfered. On reperusing his letter, I perceive that he notices a disagreement between my figures and those given by Mr. Symons, which requires to be explained, and I take the opportunity of endeavouring to remove his doubts with regard to the correctness of my results. Mr. Symons's annual averages for 1843–61 were drawn from one station, or rather from two; from Fairfield for the years 1843–46, and from Halton, a station nearly three miles distant, and having twice the elevation, for the rest of the period. My averages were taken for the first four years from the same single station, the only record then in existence, and from a varying number of stations during the other years.

Mr. Meldrum thinks that, with certain alterations which he suggests, my calculations will support his theory. I should be very glad if they did. My object in pursuing my inquiries into the rainfall of Barbados has been to assist the planters in forecasting the coming seasons, so as to guide them in their agricultural operations; and I would gladly welcome every contribution to this end, whether it be Mr. Meldrum's sun-spots or Prof. Chase's lunar influences. I was therefore disappointed when I found that the experience of this island did not coincide with that of Mauritius, and I am sorry that a further comparison of the data, which is not open to any objection of discordance of elements, confirms my first calculations.

If I take Fairfield and Halton alone, for the thirty-one years 1843–73, I obtain the following results:—

	Maximum years.	Minimum years.
1843–45 ...	... —	163·7
1847–49 ...	... 158·3	—
1855–57 ...	... —	170·7
1859–61 ...	... 186·6	—
1866–68 ...	... —	177·8
1870–72 ...	... 157·1	—
Total ...	502·0	512·2

This calculation shows an annual average excess in *minimum* years of 3·4 inches. But the rainfall at Fairfield during the last three years, for which alone I have the means of comparison, is 13·33 per cent. below that of Halton. Therefore 21·7 inches have to be added to the minimum average of 1843–45, which would increase the above excess to 10·6 inches. If Halton alone be taken for the five periods, the average of the maxima is 167·3, and that of the minima 174·2, yielding an excess of *minima* of 6·9 inches.

A comparison of three stations for 19 years, 1855–73, being the longest comparable period, exhibits the same results. These three stations, Halton, Binfield, and Husbands, are situated in opposite parts of the island, and furnish a fair average of the whole:—

	Maximum years.	Minimum years.
1855–57 ...	... —	192·7
1859–61 ...	... 193·6	—
1866–68 ...	... —	182·6
1870–72 ...	... 162·7	—
Total ...	356·3	375·3

This calculation shows an annual average excess of 9·5 inches in *minimum* years, which differs only by 1·1 inch from the above corrected calculations founded on the returns of a single station.

Mr. Meldrum, in his letter of September, writes, that I have "taken 1846 and 1871 as middle maxima years [in my first paper I also took 1848], whereas 1849–72 are probably more correct." Mr. Meldrum is in error as to my having taken 1846 as

a middle maximum, as a reference to my former letter (NATURE, vol. viii. p. 245) will show; and I do not find any reference to 1846 as a maximum in Prof. Tyndall's letter, or in that of Mr. Symons, which alone I had seen when I last wrote. In both of these 1848 is named, and I demur to the changes to 1849 and 1872; to the first because, apparently without any sufficient reason, a dry year (48·10 inches) is discarded, and a wet year (67·88 inches) is added, and to the second, not because it affects my calculations, but because no reason is given. The change appears to favour Mr. Meldrum's views, but it scarcely does so, because the estimated quantity of 65 inches in 1873 resulted in an actual average of only 51·26 inches, which would make a difference of 13·74 inches in that year, and would change the trifling excess of 2·64 inches on the maximum side into a larger excess of 11·10 inches on the minimum side.

It is unnecessary, however, to go beyond the calculation which I have above submitted to show that Barbados does not bear out Mr. Meldrum's theory. I am quite prepared to agree with him that, if the preponderance of evidence drawn from a wider area and from longer periods does support it, the opposite results obtained in Barbados, although it is most favourably situated for observations of this nature, being fully exposed to the trade winds blowing over the Atlantic during the greater part of the year, and not apparently subject to any disturbing influences, only show that no particular locality can draw a safe inference as to the manner in which the presence or absence of sun-spots is likely to affect it.

A further consequence presents itself to my mind. It appears to me that the atmospheric influences entering into this question —chiefly evaporation and rainfall—must balance one another pretty equally over the face of the globe, either contemporaneously or by seasons; that the excess of rain received by some places has been drawn from others, which have consequently experienced the opposite effects of evaporation and drought. If therefore certain solar influences, whose presence is indicated by the appearance of sun-spots, have the effect of causing an excess of rain in certain years over so wide an area as Mr. Meldrum supposes, whence does this excess come? If from some atmospheric reservoir, independent of the globe, the excess would be general; the alternations of rain and drought might vary by years or by seasons, more or less long, but not contemporaneously by, or in, localities. If, however, they be drawn from the earth, or from atmospheric strata near the earth, there must be evaporation and drought in those parts whence the excess is drawn. Barbados, as I have pointed out, is singularly free from local influences which would affect its rainfall differently from the rest of the globe. When therefore I find the experience of Barbados differing from that of Mauritius, and of many other parts of the world, I am driven to the conclusion that the influences indicated by the existence of sun-spots are not universal, although they may possibly operate on, and intensify, other influences already existing from other causes; and that the absence of those influences and the existence of different effects in Barbados is not an exceptional result, but a necessary consequence, to be expected in other parts of the globe also, and to be anticipated from the ordinary operation of known physical laws. I shall not, however, be dogmatic on the point, and shall hail further proof of the correctness of Mr. Meldrum's theory as a welcome contribution to the "Meteorology of the future."

RAWSON W. RAWSON

Care of Rabbits for their Dead

SEVERAL months ago you published, among others, a letter of mine, on the "care of monkeys for their dead." Since then I have been making observations upon a similar attention displayed by rabbits, although the considerations which lead to its exercise are apparently much more practical than in the case of monkeys.

Most people are aware that if a rabbit is shot near the mouth of its burrow, the animal will employ the last remnant of its life in struggling into it. Having several times observed that wounded rabbits which had thus escaped appeared again several days afterwards above-ground, lying dead a few feet from the mouth of the burrow, I wished to ascertain whether the wounded animals had themselves come out before dying—possibly for air, —or had been taken out after death by their companions. I therefore shot numerous rabbits while they were sitting near their burrows, taking care that the distance between the gun and the animal should be such as to ensure a speedy, though not an immediate, death. Having marked the burrows at which I shot rabbits in this manner, I returned to them at intervals for a fortnight or more, and found that about one half of the bodies appeared again on the surface in the way described. That this reappearance above-ground is not due to the victim's own exertions, I am now quite satisfied; for not only did two or three days generally elapse before the body thus showed itself—a period much too long for a severely wounded rabbit to survive,—but in a number of cases decomposition had set in. Indeed, on one occasion scarcely anything of the animal was left, save the skin and bones. This was in a large warren.

It is a curious thing that I have hitherto been unable to get any bodies returned to the surface, of rabbits which I *inserted* into their burrows *after death*. I account for this by supposing that the stench of the decomposing carcase is not so intolerable to the other occupants of the burrow, when it is near the orifice, as it is when further in. Similarly, I find that there is not so good a chance of bodies being returned from an extensive warren of intercommunicating holes, as there is from smaller warrens or blind holes; the reason probably being, that in the one case the living inhabitants are free to vacate the offensive locality, while in the other case they are not so. Anyhow, there can be no reasonable doubt that the instinct of removing their dead has arisen in rabbits, from the necessity of keeping their confined domiciles in a pure condition. GEORGE J. ROMANES

Dunskaith, Ross-shire, July 26

THE NEWFOUNDLAND SEAL FISHERY*

THE vessels employed in this fishery are generally built for the purpose at Aberdeen, Greenock, or Dundee; but some obsolete men-of-war have been bought and strengthened to meet the requirements of the trade. Those steamers built for the purpose range from 170 to 470 tons register, and have screw propellers. The *Bear*, in which I went, belonging to Messrs. Walter Green and Co., and commanded by Captain Alexander Graham, a sealing master of thirty years' experience, was a new vessel of the largest class, built by Messrs. Stephens, of Dundee, was barquentine rigged, and had compound engines of 110 H.P.

The smallest rod in the latter was 2½ inches in diameter, the minimum that has been found to stand the shock of concussion with the ice. Propellers are made in one piece of cast-iron; metal having been tried was found to twist, and those made with separate blades to screw in inevitably broke in the thread of the screw. They are about 7 in. in thickness near the boss and about 2 in. at the point, and should be made without a sling hole, two propellers of the *Bear* having broken at that place. Over the banjo frame are the "slip boards," pieces of hard wood about 3 in. thick, that slide down the screw well on each side of the Sampson posts to prevent ice getting in above the propeller. They should be made to hoist up in one piece with the banjo, otherwise considerable time is lost in unbolting them. The brine from salt-meat casks is kept and poured down boiling to loosen the gear set fast by frost and ice. The propeller may be known to be broken by the great increase in vibration that inevitably follows when in the ice. After watching for a long time I found the effect produced on the engines by the ship striking the ice was scarcely perceptible, and the stoppage of the propeller by ice even at full speed only caused the connecting rod to vibrate slightly.

The bows for about 20 ft. from the stem are built nearly solid with the numerous beams, timbers, and diagonals; this space is called "the fortification." The bows are sharply built with a raking gripe, the advantage of which is that the vessel does not strike the ice on all the stem at once, but gradually meets the pan, and by the force of the way runs on it as up an inclined plane, and thus adds weight to momentum in breaking a passage. The stern should be

* The following notes from personal experience were made in the present year by Navigating Lieutenant Wm. Maxwell, R.N., and communicated to the Hydrographer of the Admiralty.